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(University of London),
21 Torrington Square,
LONDON, W.C.1.

Dr. C.A. Knight,
The Virus Laboratory,
University of California,
U.S.A.

15th November, 1954.

Dear Dr. Knight,

Since I got back I have been going over my conclusions about T.M.V. somewhat more critically in the light of what I learned from you and others during my visit to America. I enjoyed discussing these things with you very much and I am particularly grateful to you for the copy of your review article on the Chemical Constitution of Viruses, which contains a great deal of valuable information for me. There is much reference in it to work that is as yet unpublished, and there are one or two things I should like to ask you about this. You give the number of threonine end group as 26,000 per 50 million molecular weight, instead of 34,000 as in your published work. Is it certain that 34,000 is wrong? It fits better with my X-ray data according to my present ideas. I think the balance of evidence is in favour of 37 residues in 3 turns of the helix, giving a molecular weight of 29,000. And I like to think that this sub-particle consists of two more-or-less equivalent halves, of M.W. 14,500. Do you consider your estimation of cysteine to be sufficiently accurate to eliminate the possibility of the lower figure for the molecular weight? Looking at the values you give for other amino-acids present in small quantity (particularly lysine, histidine and tryptophane) it seems that there cannot be a single type of truly identical sub-particle of molecular weight as small as this. Would you suggest that two given amino-acids may be interchangeable within the sub-particle? This would account for the possibility of having strains with no detectable difference of composition - they might simply have a different spatial distribution of the interchangeable amino-acids.

I should be most grateful if you would let me know what you think about these questions.

I am in the process of writing up a short note on my work, and I shall naturally want to refer to some of the chemical work. May I refer to your article as "Advances in Virus Research, Vol.2" or would you rather I made the reference less precise, or else confine my references to what has already appeared in print? Could you tell me where the Fraenkel-Conrat and Singer (1954) article is to be published?

My X-ray equipment is now working again and I should very much like to examine some of your materials when you have any to spare. In particular I think it would be useful to look at the cucumber virus to try to find out in what ways it differs structurally from T.M.V.

Yours sincerely,

Rosalind Franklin